Crick structure, in particular, if the net flow of liquid is inward. It is interesting to note the value obtained for T5 in this case; it is of the order of a few minutes. Luria and Steiner (*13*) found the injection time for T5 to be several minutes. They suggested that the narrowness of the tail might be responsible for the rather slow penetration of the phage DNA. In our model, the length of this structure would appear to be equally important.

In proceeding, we wish to mention that one should perhaps not exclude the possibility of a stretching of the DNA subsequent to irreversible attachment and possibly enzymic liquefaction of part of the interior of the phage. It is well known that the length of Watson-Crick structure B, which represents purified DNA at high relative humidity, exceeds by 30 percent that of structure A obtained at a lower humidity (*5*). If a similar stretching does take place, it may cause part of the structure to be pushed into and partly through the tail. This would mean that our mechanism would need to be active for a shorter period of time, thus reducing the values presented in Table 2.

It remains to be considered whether partial entry into the bacterium changes the conditions radically enough for the foregoing model to become meaningless. In point of fact, it is conceivable that the process will be speeded after partial entry. Qualitatively, we may say that uncoiling and entry amount to an increase in entropy. Again, if biochemical reactions involving a reduction in free energy are initiated already by partial entry, the effect will amount to a pulling force. Similarly, the greater freedom of sidewise motion of the part of the thread pointing into the bacterium permits oscillations that give rise to a net centrifugal force. To get an idea of the size of this effect we can consider that a fraction λ of the thread is capable of such oscillation in a plane. Giving it a mean oscillatory kinetic energy of $\frac{1}{2}\ kT$ (thermal energy) we shall take this to be the instantaneous energy $E=\frac{1}{2}I\omega^2$, where I is the moment of inertia about the point of entry and ω is the angular velocity. In terms of mass (μ) and length, we have $I=1/3\mu\lambda^2$, while the centrifugal force is $F=\frac{1}{2}\mu\lambda\omega^2$. Thus,

$$F=\frac{3E}{\lambda}=\frac{3KT}{2\lambda}$$

We conclude that, to the extent that these considerations apply, the force may accelerate the entry appreciably. Thus, if we insert in this formula a value for λ of, say, 100 A, the corresponding value of F is of the order of 10^{-7} dyne. The corresponding velocity (v) would be B times this quantity—that is, of the order of 10^{-2} cm/sec. If, more generally, the expression for F were valid during entry, the time taken for it to effectuate the process would be given by

$$t=\int dt=\int\frac{d\lambda}{v}=\int\frac{d\lambda}{FB}=\frac{l^2}{3kTB}$$

(assuming a constant B, compare preceding discussion). Hence, such an approach to the problem of entry would lead, essentially by itself, to durations of the process that would roughly equal those found by the preceding treatment based on the assumption of Brownian motion. To some extent, a centrifugal pull will occur which will stimulate entry; it would appear to be somewhat unrealistic, however, to neglect completely any freedom of transverse motion in the tail and also to employ the afore-stated expression for F throughout a large range of values of λ.

Although we definitely do not wish to claim that we have presented *the* mechanism for the entry of nucleic acid through the tail of a virus into the host bacterium, we do feel that the main mechanism proposed, which should take place in any case, together with the supplementary ones, offers a good possibility of explaining the method of entry. It should be noted that, since B depends on $1/\eta$ and t on $1/BT$, the time of entry should be temperature-dependent according to the factor, viscosity/temperature. If a method of measurement of the time of injection should become more practicable, this relationship could perhaps be verified to some extent. Likewise, observations with partial replacement of cellular water by a more viscous solvent (*14*) deserve consideration.

References and Notes

1. T. F. Anderson, *Am. Naturalist* **86**, 91 (1952) and *Cold Spring Harbor Symposia Quant. Biol.* **18**, 197 (1953).
2. A. D. Hershey and M. Chase, *J. Gen. Physiol.* **36**, 39 (1953); A. D. Hershey, *Virology* **1**, 108 (1955); R. M. Herriot, *J. Bacteriol.* **61**, 752 (1951).

3. This study was aided by a grant from the John A. Hartford Foundation. We wish to acknowledge helpful discussions with H. Rappaport and other members of the biophysics department of Yale University.

4. G. S. Stent and C. R. Fuerst, *J. Gen. Physiol.* **38**, 441 (1955).
5. J. D. Watson and F. H. C. Crick, *Cold Spring Harbor Symposia Quant. Biol.* **18**, 123 (1953).
6. E. Pollard, *Advances in Virus Research* **2**, 134 (1954); M. Davis, *Arch. Biochem. and Biophys.* **49**, 417 (1954); E. Pollard and J. Setlow, *Radiation Research* **4**, 87 (1956).
7. R. C. Williams and D. Fraser, *J. Bacteriol.* **66**, 458 (1953).
8. A. Garen and T. T. Puck, *J. Exptl. Med.* **94**, 177 (1951).
9. A. Einstein, *Ann. Physik.* **19**, 371 (1906).
10. L. Hopf, *Handbuch Physik* (Springer, Berlin, 1927), vol. VII, p. 95.
11. T. T. Puck and H. H. Lee, *J. Exptl. Med.* **99**, 481 (1954) and **101**, 151 (1955).
12. A. Ore and E. Pollard, *Bull. Am. Phys. Soc.*, No. 1 **31**, 200 (1956).
13. S. E. Luria and D. L. Steiner, *J. Bacteriol.* **67**, 635 (1954).
14. H. J. Morowitz, *Arch. Biochem. and Biophys.* **59**, 341 (1955).

Science and Freedom

Freeman Dyson

I spent 2 weeks in May of this year going to scientific meetings in Moscow, talking with Russian physicists and sitting in Russian laboratories. A dozen Americans and many other foreigners were there. All of us reported, after we came home, that we were astonished at the enthusiasm, the competence, and the solid achievements of the Russian scientists.

Now, the editor of the *Sun* asks me a question. He says, "It is commonly stated by men of science that freedom is essential to a healthy scientific climate. And yet we learn from members of your group that Russian science, which has surely had to put up with security arrangements more stringent than ours, is in a flourishing condition, and that Russian scientists show evidence of the highest morale in their personal and scientific life. How can this be so?" He invites me to set down my thoughts about this question. And I am happy to do so, because

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the question is a real and important one. If science is to continue to flourish in our country, we scientists must succeed in making our needs understood by the public. For this reason, I welcome every chance to explain our situation, and especially to explain the nature of the challenge being offered to us by Russia.

Not Yet as Good

First of all, let it be clearly said that Russian physics is not yet as good as American physics. We were amazed at their work, not because it is so wonderful in itself, but because it has improved so much so fast. We felt like Dr. Johnson, when he heard a woman preach and delivered his famous judgment: "A woman's preaching is like a dog's walking on his hinder legs. It is not done well; but you are surprised to find it done at all." Some of our group had worked in Russian laboratories before the war. In those days almost every piece of equipment more complicated than a screwdriver was imported, mostly from Germany. Those gadgets which were made in Russia were not expected to function.

Today, all that has changed completely. The Russians know how to make scientific equipment, equal in quantity and quality to any in the world, and they have plenty of people who know how to use it. It is the speed and suddenness of their progress which are impressive. What they have done with their equipment is not yet so exciting. Since the war, six first-class and revolutionary experiments have been done in physics. Of these, one was done in Italy, one in England, and four in the United States. None so far in Russia.

The second main fact which we established beyond doubt was this. Russian work in physics is now essentially free. I am not speaking here about political freedom. This, of course, does not exist in Russia, and will not exist in the foreseeable future. But a reasonable scientific freedom does now exist. That is to say, Russian physicists enjoy the basic professional freedoms, to work on problems of their own choosing, to publish their results, and to discuss their ideas with foreign colleagues. These freedoms are restricted by security rules which are similar to ours, perhaps slightly stricter. In my own conversations, I found the only subject the Russians were unwilling to talk about was the construction of their latest electronic computers. This subject was clearly "classified" for them, although it is "unclassified" for us. On the other hand, they could talk about some experiments relating to thermonuclear reactions, which for us are still classified.

Freedom Is New

The freedom of Russian science is quite new. It came suddenly, soon after the death of Stalin. Until 2 years ago, nothing whatever was published of experimental work in nuclear physics. There was no possibility of personal contacts with foreign scientists, even from the satellite countries. And a high proportion of the physicists were engaged in military work. Two years ago, the whole atmosphere changed. People poured back from the military projects into pure science, publication was encouraged, and international meetings allowed.

All this had an intoxicating effect on Russian scientists. Suddenly to be given these freedoms, which they had not known for 15 years, filled them with optimism and self-confidence for the future. All the time we were in Russia, we could feel how happy they were to be allowed to talk to us. Their enthusiasm and high morale are directly caused by their new experience of freedom. The superstrict security system of Stalin's time produced high morale only in this negative way, like the man in the lunatic asylum who continually beat his head against a brick wall because it felt so good when he stopped.

The good experimental work which has been done in Russia was done after the new regime began. We found clear evidence that the different laboratories in Moscow had been isolated from each other during the earlier time, and that this had hampered their work considerably. For example, the big cyclotron in Moscow had been working since 1949, and was for several years a better machine than any working in this country. But nobody with imaginative ideas for new experiments had access to it, and so the basic experiments which established the properties of the meson were all done in America.

It is clear that the Soviet Government now understands the fact, which the American Government always knew, that scientific progress demands scientific freedom. It is also clear that the Soviet Government is spending enormous amounts of money on pure science, and seriously intends to make Moscow the scientific capital of the world. They have understood that the power of American science depends on America freely and openly attracting people and ideas from all over the world. And they intend now to beat us at our own game.

Public Support

I will end this discussion with one little story. After the meetings were over, a group of foreign scientists with two interpreters went sightseeing in the country around Leningrad. We walked by mistake into some kind of coast-guard station, evidently a restricted area, but nothing of importance. An ordinary Russian seaman came out to shoo us away, shouting, *"Nelzya,"* which means "forbidden." At the same time, we noticed that our interpreters, evidently unwilling to be held responsible for this error, were walking rapidly away in the opposite direction.

So we stayed and had a friendly chat with the seaman in our broken Russian. When I said we were foreign scientists, he immediately said, "Oh, I know who you are. You are the people who have been at the meeting in Moscow, and you know all about pi-mesons and mu-mesons." He pulled out of his pocket a crumpled copy of *Pravda* in which there was a report of our proceedings. He talked then with great warmth, saying, "Why do you not come to our country more often?" and, "Be sure to tell the people in your countries, and your wives and children, that we would like to see more of them."

I am fairly sure that this sailor had not been planted, briefed, or warned beforehand of our coming. If it had been a plant, the interpreters would not have walked away. And in Moscow I talked with several other nonscientific Russians whom I met casually in the street, and they all made the same kind of response to me.

The moral of this story is that the ordinary Russian people have an understanding of the value and importance of pure science. And they understand and take pride in the fact that learned foreigners come to their country to exchange ideas. It is the atmosphere of public understanding which makes the prospects for the future of science in Russia look so bright. Their scientists have a professional freedom which is not much less than ours, and they have a public support which is in some ways much greater. I can only hope that an American coastguard sentry, confronted unexpectedly with a group of Russian physicists speaking broken English, would have behaved with equal intelligence and respect.

First get your facts; and then you can distort them at your leisure—MARK TWAIN.